the fact that clouds are not distributed promiscuously at all heights in the air, but that they have, on the contrary, a most decided tendency to form at three definite levels. The mean summer level of these three stories of clouds at Upsala has been found to be as follows: low clouds—stratus, cumulus, cumulo-nimbus, 2000–6000 feet; middle clouds—strato-cirrus and cumulo-cirrus, 12,000–15,000 feet; high clouds—cirrus, cirro-stratus, and cirro-cumulus, 20,000–27,000 feet.

It would be premature at present to speculate on the physical significance of this fact, but we find the same definite layers of clouds in the tropics as in these high latitudes, and no future cloud-nomenclature or cloud-observations will be satisfactory which do not take the idea of these levels into account.

But the refinements of the methods employed allow the diurnal variations both of velocity and altitude to be successfully measured. The velocity observations confirm the results that have been obtained from mountain stations—that, though the general travel of the middle and higher clouds is much greater than that of the surface winds, the diurnal variation of speed at those levels is the reverse of what occurs near the ground. The greatest velocity on the earth's surface is usually about 2 p.m.; whereas the lowest rate of the upper currents is about midday.

The diurnal variation of height is remarkable, for they find at Upsala that the mean height of all varieties of clouds rises in the course of the day, and is higher between 6 and 8 in the evening than either in the early morning or at midday.

Such are the principal results that have been obtained at Upsala, and no doubt they surpass any previous work that has been done on the subject. But whenever we see good results it is worth while to pause a moment to consider the conditions under which the work has been developed, and the nature and nurture of the men by whom the research has been conducted. Scientific research is a delicate plant, that is easily nipped in the bud; but which, under certain surroundings and in a suitable moral atmosphere develops a vigorous growth.

The Meteorological Institute of Upsala is an offshoot of the Astronomical Observatory of the University; and a University, if properly directed, can develop research which promises no immediate reward in a manner that no other body can approach.

If you want any quantity of a particular kind of calculation, or to carry on the routine of any existing work in an Observatory, it is easy to go into the labour market and engage a sufficient number of accurate computers either by time or piece-work, or to find an assistant who will make observations with the regularity of clockwork. But original research requires not only special natural aptitudes and enthusiasm to begin with, but even then will not flourish unless developed by encouragement, and the identification of the worker with his work. It is rarely, except in Universities, that men can be found for the highest original research. For there only are young students encouraged to come forward and interest themselves in any work for which they seem to have special aptitude.

Now, this is the history of the Upsala work. Prof. Hildebrandsson was attached as a young man to the meteorological department of the Astronomical Observatory, and when the study of stars and weather were separated, he obtained the second post in the new Meteorological Institute. From this his great abilities soon raised him to the directorship, which he now holds with so much credit to the University. M. Ekholm, a much younger man, has been brought up in the same manner. First as a student he showed such aptitudes for the work as to be engaged as assistant; and now, as the actual observation and reduction of the cloud-work is done by him and M. Hagström, the results are published under their names, so that they are thoroughly identified with the work.

Upsala is the centre of the intellectual life of Sweden, and there, rather than at Stockholm, could men be found to carry out original research. It redounds to the credit of the University that it has so steadily supported Prof. Hildebrandsson, and that he in his turn has utilized the social and educational system by which he is surrounded to bring up assistants who can co-operate with him in a great work that brings credit both to himself, to themselves, and to the Institute which they all represent.

RALPH ABERCROMBY.

A NEW COSMOGONY[1]

I.

THE volume before us is composed of a series of essays, first published in the Catholic periodical, *Natur und Offenbarung*, in 1885–86. By far the greater part of it is, nevertheless, of a purely scientific character. The opening chapter alone enters upon theological considerations, which we cannot here pretend to discuss, recommending merely, in passing, the broad and wise doctrines it contains to the notice of those well-meaning persons who apprehend danger to creeds from speculations as to origins.

That of our planetary system is very actively in debate just now. The nebular hypothesis, as fashioned by Laplace, no longer fits in with all the known facts. There are so many of them that it would be surprising if it did, since the outside of its claim was to the plausible representation of possible truth. It had a part to play in the history of science, which it played with eminent success. This was to show that thought, safeguarded by right reason, might be trusted to run backward towards the beginnings of things—that, without visible discontinuity or breach of known laws, the present fair scheme of creation might have emerged from the brooding darkness of chaos, along paths not wholly inaccessible to human discursive faculties.

But now the reiterated blows of objectors may fairly be said to have shattered the symmetrical mould in which Laplace cast his ideas. What remains of it is summed up in the statement that the solar system did originate, somehow, by the condensation of a primitive nebula. The rest is irrevocably gone, and the field lies open for ingenious theorizing. It has not been wanting.

The newer cosmogonists are divided into two schools by the more or less radical tendencies of the reforms they propose. Some seek wholly to abolish, others merely to renovate, the Kant-Laplace scheme. The first class is best represented by M. Faye, the second by M. Wolf and Dr. Braun. Dr. Braun is, however, a more thorough-going renovator than M. Wolf. The edifice, as "restored" by him, shows, indeed, little trace of its original aspect. Scarcely the invisible foundations are the same; the superstructure is unrecognizable. We will endeavour to sketch its main features.

In widening the nebular hypothesis to embrace the whole sidereal world, our author demands as little as possible in the way of postulates: simply a co-extensive nebula, structureless, motionless, tenuous, its particles endowed with gravity and atomic repulsion. Such a nebula, if perfectly homogeneous, should give birth to one portentous, solitary sun. But, in point of fact, it would possess innumerable, almost imperceptible, local irregularities, which, forming so many centres of attraction, would eventually lead to the breaking-up of the nebula into a vast multitude of separate fragments.

On one of these, the destined progenitor of the solar

[1] "Ueber Cosmogonie vom Standpunkt christlicher Wissenschaft. Mit einer Theorie der Sonne." Von Carl Braun, S.J. (Münster: Aschendorff, 1887.)

system, we are asked to concentrate our attention. The manner of its development is, however, a widely different one from that traced by the great French geometer. Laplace *assumed* the needful rotation, and left the rest to work itself out spontaneously. He permitted no external interference with the tranquil processes which he discerned as progressing through the ages. Dr. Braun, on the other hand, assumes less to begin with, but invokes adventitious aid in emergencies. No single nebula thrown on its own resources sufficed, he finds, for the production of the solar system. The complicated phenomena which it presents demand a complex origin to explain them. The mass of cosmical matter in which they first began to unfold themselves was accordingly but the nucleus of what it afterwards became. It not only grew by the accretion of similar masses falling towards it from space, but acquired its gyratory movement by eccentric collisions with them. The consequences of such events elsewhere are visibly pictured to us in the spiral lines of light of certain nebulæ. The great whirlpool in the Canes Venatici, for instance, betrays and records the fall of a comet, on the gigantic primitive scale, into an embryo sun. Only thus, in our author's opinion, can the strange peculiarities of its structure be accounted for; and only thus can the first impulse to axial rotation in our own system have been given.

The visits of comets, as we now see them, feebly represent, we are told, the colossal in-rushes from interstellar regions by which the machinery of planetary production was set going and modified. But it is difficult to allow to such bodies the independent origin implied in the claim for them of such illustrative significance. Comets can no longer be set down as mere casual intruders upon the solar system. They certainly share its translatory motion, since, if they were either overtaken or encountered, they should seem to come most numerously from near the apex of the sun's path. But they approach him indifferently from all parts of the sky. A further proof of the absence of relative motion is afforded by the shape of the tracks they pursue. M. Faye has remarked that, of 364 cometary orbits calculated, not one is a decided hyperbola ("Origine du Monde," p. 146); and Laplace's view that they are hyperbolic by nature, and elliptical only through perturbations, is thus seen to be the exact reverse of the truth.

A fundamental objection to Laplace's cosmogony is that it implies a far swifter axial movement in the central bodies of our system than they actually possess. For in the theory of annular separation, the rotation of the generating mass is strictly correlated with the revolution of its offspring by the principle of the conservation of areas, which requires that a rotating homogeneous globe should spin quicker, as it contracts, in the proportion of the square of its radius. Thus, if the solar nebula, when it filled the orbit of Neptune, rotated (as on the hypothesis in question it must have done) in Neptune's period of 165 years, the period of the sun shrunken to its present dimensions, should have shortened in the ratio of the square of 2,780,000,000 (the mean distance of Neptune) to the square of 434,000 miles (the solar radius). In other words, the rotation of the sun should be accomplished in 127 seconds, in lieu of 25 days. Similarly, the terrestrial rotation-period corresponding to the lunar revolution in $27\frac{1}{3}$ days, is no more than $10\frac{4}{5}$ minutes! It is true that in both these estimates (the latter taken from Dr. Braun's pages), the effects of central condensation are neglected, although it must inevitably have made some progress before annulation began; but no allowance on this score, however liberal, can possibly reconcile, though it contributes to lessen, the discrepancy.

Dr. Braun adjusts the balance in this way. The solar nebula had never at any time, in his view, a uniform axial movement. He even ventures to consider the present unequal rotation of the sun as a survival of the primitive state of things to which the central deficiency of rotational momentum is due. For the entire mass was, in the beginning, set gyrating by external impacts. Movement was hence generated predominantly in its outer regions, and was only by degrees and imperfectly communicated to the nuclear parts.

The device is marked by considerable ingenuity, and is at any rate preferable to the eddying movements by which M. Faye evades the same embarrassment. It is, however, scarcely needed by Dr. Braun, since the "ring-theory" of planetary formation is almost, and logically ought to be completely, abandoned by him. Difficulties have of late been thickening round it; they reached a climax when the conviction was attained that, apart from the neutralizing effects of tidal friction, it could only result in the retrograde motion of all secondary systems. The plan of centres of condensation is accordingly substituted by our author. This has the advantage of allowing planets to begin to form anywhere in the nebula. It emancipates them from that strict conformity to the equatorial level which was an inconvenient feature of Laplace's hypothesis; and though they necessarily tended, in the course of their growth, to descend towards it, enough may perhaps remain of their primitive divergence to explain the observed slight deviations from the fundamental plane. Yet Dr. Braun's confidence in this *rationale* of the inclinations of the planetary orbits is so far from being unlimited that he holds in reserve, in case of its failure, other means for bringing about the same end.

Each planet is roughly estimated to have started on its career at about five times its present distance from the sun. In condensing, it descended towards it, sweeping up materials as it went, until finally almost the whole of the diffused gaseous stuff was concentrated in sun and planets, and the intervening spaces were void. By that time, too, tangential velocity had come to balance gravity, and the slow inward approach ceased. But the resistance met with in the earlier stages of its history by the growing and falling planet had had one result of vital importance to its future. It had imparted to it a movement of rotation. As it settled down in close spirals towards its present orbit, its velocity must everywhere have exceeded, by a small amount, the velocity in the same direction of the medium in which it moved. The density of that medium must, however, have increased towards the sun; and the embryo-planet consequently experienced a slight excess of resistance on its inner side, resulting in a direct whirling movement. Dr. Braun endeavours to show that the rotation thus set on foot must have belonged chiefly to the external layers of the planetary nebula. His motive is that of conciliating the swift circulation of satellites yet to be born from it with the comparatively sluggish spinning of the parent mass.

Tidal friction he rejects as an agent of planetary development, attributing to it barely the power to have rendered absolute an already approximate coincidence between the periods of rotation and revolution of satellites. Perhaps he might here be induced to reconsider his position. At least in the case of the lunar-terrestrial system, the evidence is overwhelming that tidal friction was largely concerned in bringing about its present condition. We may further assure him that Prof. G. H. Darwin (whom he evidently identifies with the late Charles Darwin, his father) has not committed the blunder he imputes to him of ascribing to the moon a *shorter* period of revolution than that of the earth's rotation, at the time when it began, under the reactive influence of the tidal wave, to travel slowly outward from near its surface. On the contrary, a slightly inferior angular velocity in the satellite is the assumed starting-point of all his subsequent reasoning on the subject.

For the completion of the solar system in its minor details, Dr. Braun is driven to the expedient of collisions with some of the many nebulous fragments which continued to be drawn towards it from unfathomed depths of space. Most of these became incorporated with the sun, but a certain proportion must have been intercepted by the planets, which, in their forming state, as possessing less mass and velocity, were more sensitive to such shocks than when fully formed. Thus, the plane of the ecliptic might have been altered, we are told, 1° by the impact upon the inchoate earth of a body possessing 1/1000 its present mass. Facilities even greater were offered for changing the elements of rotation; that of the earth, when of seventy times its actual radius, might even have been stopped altogether, by collision, under specially favourable circumstances, with a mass only 1/10600 the terrestrial.

But this method of explanation is radically unsatisfactory. It suggests the *Deus ex machinâ* of an unskilled dramatist, and cannot be admitted without protest into scientific speculation. We have learned to regard cometary impacts as the last resource of the distressed cosmogonist. Such events are not impossible, but to resort to them in difficulties is to throw up the game of ordered inference. The conviction remains unalterable that the results visible to us were brought about by means less apparently fortuitous. Dr. Braun, for example, is obliged to suppose not only that, before the separation of the moon, the axis of the lunar-terrestrial nebula was deviated, by extraneous agencies of the kind indicated, to the extent of 5° from its original position of perpendicularity to the plane of the ecliptic; but that, subsequently to the separation, further shocks continued the process upon the earth alone until the inclination attained its present value of 23½°. Still less admissibly, the solar axis is assumed, after the formation of Venus, to have been tilted 5° by a number of successive impacts. A transcendent degree of improbability seems to be reached by this conjecture.

In the order of planetary production, Dr. Braun follows Laplace. Neptune is his oldest planet. And the fact that it revolves very nearly in the "invariable plane" of the solar system is confirmatory of the view that it really was the first body (instead of being the last, as M. Faye supposes) to become severed from the primitive nebula, the rotation of which is likely to have been conducted in that plane (Wolf, *Bull. Astronomique*, t. ii. p. 228). Neptune alone, owing to the distinction of his retrograde rotation, is allowed by our author to have been formed by the detachment and eventual condensation of a nebulous ring. But Prof. Kirkwood has raised an objection to this orthodox mode of genesis which applies with especial force to the remotest planet. The coalescence into a single globe of the fragments of a broken-up ring, if it happened at all (which is uncertain) would, it appears, have been an unconscionably slow process. Thus, two opposite portions of a ring of the dimensions of Neptune's orbit, could scarcely have come together in less than 150,000,000 years. It must be admitted that this is a startling demand on the time-exchequer even of the cosmos.

Uranus is regarded by Dr. Braun as what Bacon called a "limiting instance" between the annular and the nuclear methods of generation. An abortive ring gave place to a centre of condensation, the result (helped, perhaps, by some well-aimed cometary shoves) being the indecisive character of the Uranian rotation on an axis lying prone in the plane of revolution.

These, then, are the main outlines of the last and newest cosmogony. While dissenting from some of its conclusions, we readily admit that it is, in several ways, a noteworthy effort. Its appearance may be said to mark the definitive abandonment, by sound thinkers, of the annular method of planet and satellite formation. The preciseness of the conditions of that celebrated hypothesis lent it its charm, but has proved its ruin. Had they been less definite, it might have lived longer. But it gave, as it were, hostages to the future which it has not been able to redeem.

It is gradually becoming clear that, while the various members of the solar family owned unquestionably a common origin, they can scarcely be said to have had a common history. Each ran through a cycle of development particular to itself, and appointed, without doubt, to adapt it to a special purpose. The biography of the earth and moon, as narrated by Prof. Darwin, is an example. Here influences predominated which, in every other secondary system, were comparatively unfelt.

This growing persuasion of what we may call planetary individuality is reflected in Dr. Braun's vigorous and original chapters. He has honestly, and with no small ability, worked out *ab initio* the problems that they deal with, and he finds them insoluble by the uniformitarian method of treatment. The expedients by which he seeks to obtain a diversity of results by introducing a diversity of vicissitudes, strike us perhaps as arbitrary and awkward; but the admission of their necessity by an inquirer of such acuteness, and so well abreast of contemporary scientific thought, is highly instructive. We shall return later to the part of his interesting work devoted to solar theory.

A. M. CLERKE.

NOTES.

On Tuesday Lord Hartington introduced to Sir W. Hart Dyke a deputation consisting of Sir Henry Roscoe, Sir Lyon Playfair, Mr. Rathbone, Mr. G. Howell, Mr. Cyril Flower, Sir B. Samuelson, and other gentlemen interested in education. They urged the desirability of the Technical Education Bill being passed, and of certain changes being made in the measure. Sir W. Hart Dyke replied favourably on both points. He was of opinion that the prospects of the Bill were good, both in the House of Commons and in the House of Lords.

The list of foreign men of science who have accepted the invitation of the Local Committee to attend the Manchester meeting of the British Association is steadily increasing, and now numbers considerably over a hundred. Amongst those whose names have been received since the list we published on July 7, we note Prof. Cremona, of Rome; Dr. Neumayer, Director of the Hamburg Marine Observatory; H. A. Rowland, Baltimore; Dr. Werner Siemens, of Berlin; Prof. Horstman, Heidelberg; Prof. L. Weber, Breslau; Prof. Capellini, Bologna; Prof. Carvill Lewis, Philadelphia; Prof. O. Bütschli, Heidelberg; Prof. Carnoy, Louvain; Prof. Erb, Heidelberg; Dr. Treub, Director of the Botanic Gardens, Java; Capt. Coquilhat, Brussels; Dr. Godefroi, 's Hertogenbosch; Dr. Ludwig Wolf, Leipzig; Signor Bonghi, the late Italian Minister of Education; Signor Luzzati, Rome; Dr. E. Atkinson, Director of the Massachusetts Bureau of Statistics; and Dr. G. Boissevain, Amsterdam. The King of the Belgians has intimated his intention of nominating two representatives of the Congo Free State to attend the meeting, and of these General Strauch, Administrator-General of the Congo Free State, is expected to be one. A correspondent in Paris writes to us that the Emperor of Brazil, who has lately spent some time in the French capital, will probably attend the meeting of the Association.

Mr. A. T. Atchison, Secretary of the British Association, is authorized to state that at the Manchester meeting space will be provided in the galleries of the Reception Room for the exhibition of specimens and instruments shown in connexion with and in illustration of papers read in the Sections. To secure admission a brief description of the specimens or instruments must be submitted to the Local Secretaries not later than August 10, together with a statement of the dimensions of the table or other fixture required. No motor power will be available. The objects must be exhibited at the risk of the owners;